cists, but also by those engaged in other scientific pursuits who desire trustworthy information as to the "new physics." H. S. A.

Le Rocce. Concetti e Nozioni di Petrografia. By Prof. E. Artini. Pp. xx + 636 + Tav. xxxii. (Milano: Ulrico Hoepli, 1919.) Price 18.50 lire.

PROF. ARTINI states in his preface that there has been no general treatise on rocks in the Italian language since that by Achiardi, published thirty years ago. He rightly remarks that a translation is always an indifferent expedient; a book for Italians should be rich in Italian examples. He looks on rocks from the point of view of a naturalist, and his use of landscapes among his illustrations makes us hope that he will some day give us a petrography of Italy that will connect mineral evolution with the scenery from Monte Bianco to Catania. The material here brought together is thoroughly up to date; we may cite, for instance, the remarks on *idrogels* (p. 186), on bipyramidal quartz (p. 338), and on the alleged gneissic *Grundgebirge* (p. 544). Graphic methods of representing rock-composition are illustrated. As an Italian detail, may we point out (p. 319) that *gabbro,* and not *eufotide,* is of Tuscan origin, the name of a Tuscan village having been utilised by von Buch? The treatment of sedimentary rocks is unusually adequate, and the photographic plates of thin sections are extremely clear and helpful. This compact volume is so full of fundamental *concetti* that it certainly should have been provided with an index. G. A. J. C.

Agricultural Bacteriology. By Dr. H. W. Conn. Third edition, revised by H. J. Conn. Pp. x + 357. (Philadelphia: P. Blakiston's Son and Co., 1918.) Price 2 dollars net.

WHILE the general plan of this book remains the same as before, considerable changes have been introduced in the sections on soil bacteriology, on the control of milk supplies, on plant diseases, and on laboratory technique. In some cases, however, further information might have been given with advantage; thus under slimy or ropy bread practically no description is given of the causative organism. Under "tuberculosis" the illustration Fig. 50 is stated to depict "a bit of animal tissue"; what is actually shown is a giant cell only; the tubercles are stated to be "swollen masses of tissue," and among animals that suffer from tuberculosis dogs and cats are mentioned; actually these animals rarely suffer from the disease. The consideration of the bacteriology of the soil, of milk, and of milk products is adequate, and such details as protozoa in the soil and soil sterilisation and the possibility of the accumulation of toxic substances in "worn-out soils" are all referred to. In an appendix a scheme of laboratory work is given, with detailed exercises, which should be of value to the teacher. The book is freely illustrated and clearly printed, and forms a good elementary introduction to the wide subject of agricultural bacteriology.

R. T. H.

LETTERS TO THE EDITOR.

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts intended for this or any other part of* NATURE. *No notice is taken of anonymous communications.*]

Wireless Telephony.

REFERRING to my letter on this subject in NATURE of June 12, Mr. Godfrey Isaacs tells me that his wireless remarks with regard to secrecy were intended to apply, not to the apparatus actually in use on May 28, but to a new Marconi system, the apparatus for which is only now in course of manufacture. The scientific world will, I am sure, await with interest details of this new secret wireless telephone system.

A. A. CAMPBELL SWINTON.

40 Chester Square, London, S.W.1, June 17.

Camouflage of Ships of War.

PROF. KERR, in the course of a letter which appeared in NATURE of May 15 under the above heading, paid me a high tribute by stating that, during the summer of 1917, "the value of the principle [*i.e.* obliterative colouring] was now recognised [by the Admiralty] and its application entrusted to skilled hands," but the main point in his letter was to show that the principle of obliterative colouring was no new thing, and was common knowledge to biologists: this no one will question. My aim in replying to his letter is with the view of showing that I was not working on biological lines, and is thus to remove a misapprehension.

I feel that Prof. Kerr has not thoroughly grasped the idea of the special form of camouflage on which I was engaged, and of which I still claim to be the originator. "Dazzle-painting," so called officially, had one purpose in view only, viz. to upset a submarine commander's estimate of a vessel's course, when carrying out an attack with torpedo. I was under no misapprehension as to its value for gunnery, and in my original submission to the Admiralty in May, 1917, I made no claim that it might be used for this purpose, as I felt certain that paint could not possibly have sufficient carrying power to stultify the enemy's range-finders at the great distances at which a modern action would probably be fought.

Subsequent experiments on dazzled ships with range-finders justified this belief.

The accurate estimation of a vessel's course is the prime factor required by a submarine commander to ensure successful attack. In every dazzle design this point was studied to the exclusion of all others, *i.e.* to frustrate accurate calculation of course. The mere breaking up of a vessel's form by strongly contrasting colours would not achieve this end without careful study of the perspective and balance of the design. I am not aware that this occurs in biology, *i.e.* the disguise of direction.

Surely the obliterative colouring of birds and animals is operative only so long as the bird or animal is in a state of rest; the moment movement commences the illusion is destroyed. The ship subject to torpedo attack is in constant movement. Again, in how many cases is Nature's scheme for protection successful when the subject is seen on a ridge silhouetted against the sky? Yet this is the only point of view from a submarine when observing a ship through the periscope.

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My contention throughout has been that the degree of visibility of a vessel was of little consequence providing she could be seen at all. Prof. Kerr agrees that it is not possible to render a ship strictly invisible, but only to reduce her visibility. This in my view is not enough. A submarine commander, whose one object is to sink ships, will not be put off by reduced visibility. We know from some of the commanders themselves that they constantly located a vessel by its smoke when still hull down, *i.e.* before the vessel itself could be seen at all.

Prof. Kerr says that of the various methods which Nature makes use of, there are two alone of practical value for application to ships: (*a*) obliterative colouring; (*b*) compensative shading. I have endeavoured to show that the contrasting colours, as used in dazzle-painting, were not used in Nature's way, *i.e.* as obliterative colouring.

To turn to compensative shading, I must say, after extended observations at sea, I have failed to observe any gain in this method of painting. In a letter of this length it is not possible to go into all the causes of its failure, but only to state briefly one or two of the main objections. To take the practical side first, what shadows are there in our modern battleships to compensate which would retain white paint for more than a few hours? The various controls on the mast are in close juxtaposition to the funnel, and subject to constant heat and smoke. The hawse-pipes are rusty after a few hours' steaming, while the shadow cast by the flare of the bow is automatically compensated by reflected light thrown up from the bow wave. There is a small shelter deck amidships, far too deep shadow for any light paint to overcome.

In the case of the merchant vessel the same difficulties arise. No shadows cast by passenger decks can be overcome by the use of white paint, which is itself dependent on light for luminosity. These decks present a very different proposition from a bird's breast receiving reflected light from the ground or sand on which it stands, or from the glitter of water below. I am not theorising in making these statements; they are the direct outcome of observation at sea for some years.

There is one point I should like to emphasise in the matter of ships' camouflage, and that is, the practical application of a design to a ship. A scheme may be evolved which appears perfect on paper, but the result, when actually applied, will be most disappointing. Most theorists with whom I have come in contact—and they are many—only think in "one ship" when evolving a scheme for disguise. What has to be realised is that it is necessary to deal with hundreds of ships, painting simultaneously and at high pressure. The authorities concerned with shipping during the war could not think of any delay in unloading and getting vessels to sea in the shortest possible time. Consequently the painting of these vessels had to be carried out while loading or unloading, and under every other disadvantage, such as rain and coal dust. We were sometimes able to get a hose on to parts of a ship blackened with coal dust whilst painting, sometimes not. So that I fear so subtle a thing as compensative shading would have vanished before a vessel put to sea. It may be mentioned here that more than 3000 British ships alone were dazzle-painted in the last eighteen months of the war, and we sometimes had as many as a hundred vessels painting in one port simultaneously.

It should be remembered that dazzle-painting was adopted at a time when twenty to thirty ships were being sunk weekly, so that the life of the nation depended on turning ships round and getting them to sea again in the shortest possible time.

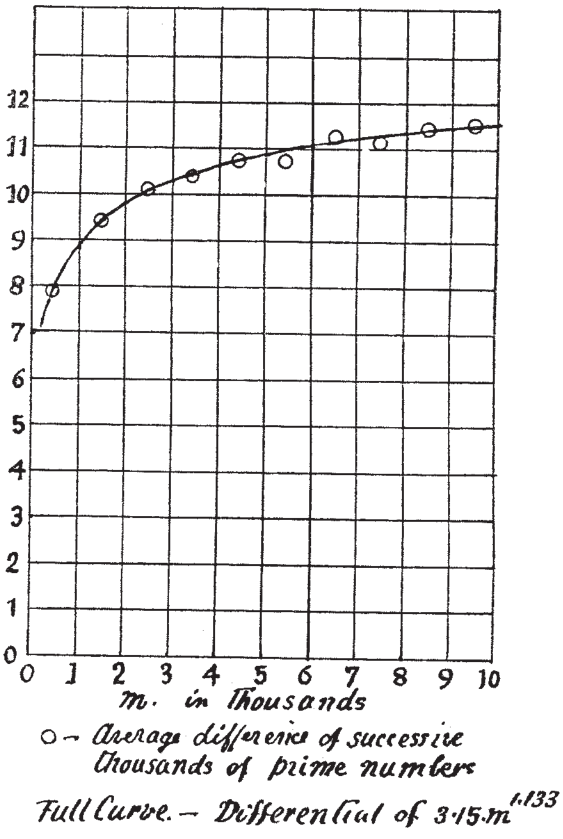
Dazzle-painting was never intended for use on "ships of the line," but only for merchantmen singly or in convoy and war vessels working with them; and, judging from the great number of reports received from merchant captains, who in the early stages of dazzle-painting were averse to it, but later came to see its object, there can be no question that it achieved its purpose.

NORMAN WILKINSON.

Question Relating to Prime Numbers.

IT is well known that no algebraical formula can represent prime numbers only, and that primes can only be found by trials (which may be facilitated by algebraical processes). If the m^{th} prime number, counting from unity, be denoted by n, and if n is plotted in terms of m, it will be found that n is very approximately represented by a formula of the type Am^p ($A=3{\cdot}15$, $p=1{\cdot}133$, are close to the values of the constants).

The differential of this curve is given in the accompanying diagram, and the true values of dn/dm are shown by the circles. The agreement between the curve $3{\cdot}15\ m^{1{\cdot}133}$ and the true values of n (taken from Barlow's tables) is too close to be shown with advantage on the scale to which the diagram is drawn. The differential curve is a good mean of the actual values of dn/dm.



Are there any investigations which give a reason for the tendency of n to approach a definite function of m, or as to the ultimate value of dn/dm when m increases without limit?

A. MALLOCK.

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